Social Psychology and the Pragmatic Conduct of Science

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ABSTRACT. We chart our agreements and disagreements with Wallach and Wallach's (2001) arguments. After listing some areas of agreement, we then make four arguments that undermine the Wallachs' project. First, we argue the discovery of 'laws' is a goal neither of scientists nor of the discipline. Second, we suggest the Wallachs' claim that social psychological theories are infused with unfalsifiable theories about the nature of psychological variables misses the logical distinction between measurement models and conceptual hypotheses. Third, we look at the role that circularity plays in deductive logic and conclude that circularity is rarely a serious problem in practice. Fourth, we examine what effect bits of research have on scientific progress, and argue that 'non-progressive' papers do not hinder progress. The best response to the Wallachs' concerns about social psychology is provided by a glance at progress within the field today. The state is very good; circularities, tautologies and untested assumptions do not pose a significant threat to progress in the science of social psychology.

KEY WORDS: circularity, scientific progress, social psychology experiments, tautology

It is a pleasure to again be afforded an opportunity to contrast our view with that of the Wallachs (Wallach & Wallach, 2001). We have, in print, disagreed with the Wallachs on the question of the meaningfulness of social psychology experiments (Schaller & Crandall, 1998; Schaller, Crandall, Stangor & Neuberg, 1995). The nature of the Wallachs' argument is increasing in sophistication, and their newest paper is perhaps their most forceful. So it is appropriate, perhaps, that we begin with a few points of agreement.

(1) We agree that social psychology, as a science, can make progress.

(2) We agree that strict social constructionism is not the only alternative to the sometimes vacuous theoretical, empirical and philosophical claims

made by social psychologists. (We can appreciate Gergen's critiques, but we do not particularly agree with them.)

(3) We agree that folk or naïve psychology is a very important component in social psychology, and it forms a substantial framework for social psychological theorizing. One of us comes from the University of Kansas, the home of Fritz Heider, whose naïve psychology ultimately formed the core of social cognition research (Heider, 1958). It is a proud tradition that we are eager to embrace.

(4) We agree that there are plenty of social psychological experiments that are conceptually dull and largely useless in propelling the science forward. (The same holds true for all other disciplines within the psychological sciences, and the sciences more broadly.)

Still, we continue to have profound disagreements about the sources of this problem and the implications of the problem for the vitality of social psychology as a discipline. We detect substantial flaws in the logical arguments employed by the Wallachs in their criticism of the field. And we have quite a different perspective as to what the objective of social psychological research is, what constitutes progress in social psychology, and what the state of the field is today.

Indeed, we are utterly optimistic about the conceptual and practical utility of social psychological research. Even if the empirical literature is littered with specific studies that accomplish little, one can argue that progress continues at the fastest pace in the history of the field.

The Structure of the Paper

We have four points to make about the logic of the Wallachs' arguments, and we will try to make them succinctly. First, we argue that the goal they project upon social psychology (the discovery of social psychological laws) is neither a goal shared by practicing researchers nor a goal of the discipline. And so a cornerstone of the Wallachs' argument is undermined. Second, we examine closely the Wallachs' claim that social psychological theories are tangled up with unfalsifiable theories about the nature of social psycho-logical variables. We argue that their claim is based on the failure to observe the logical distinction between measurement models and conceptual hypo-theses, and so is almost entirely unfounded. Third, we consider the role that circularity (or tautology) plays in deductive logic, and conclude that circu-larity is less problematic than the Wallachs imply. Fourth, we examine the role that individual bits of useless research play in scientific progress, and we argue that the impact of individual papers offering no substantial advance need not materially hinder progress of the field.

Finally, we step back from these logical arguments and suggest that the best response to the Wallachs' concerns about social psychology is provided

by a glance at contemporary progress within the field today. We argue that the state of the field is very good indeed, and so reveals very clearly that, contrary to the implication of the Wallachs' critique, circularities, tautologies and untested assumptions are not posing a significant threat to progress in the science of social psychology.

Laws of Behavior Are Not the Objective of Social Psychological Research

The Wallachs argue that circularity in social psychological hypothesis-testing interferes with the clean production of social psychological laws of behavior. This critique thus depends in part upon a particular standard for defining progress in social psychological science: the discovery of these laws. This is a peculiar way of defining progress. To some extent it assumes that progress in social psychology must be defined according to a standard adopted by some of the physical sciences, but which is rarely adopted by the biological and social sciences.

It appears that the Wallachs are adhering to a philosophical approach to scientific progress that is largely outdated and most certainly limited. This philosophy of science, exemplified by Karl Hempel (1962), indicates that scientists endeavor to create general 'covering rules'—laws that apply across situations in a regular way. This is a wonderful ideal: if one can state laws that apply across situations, one should be able to deduce what will occur in any situation. However, considerable work within philosophy of science has revealed the limits of this philosophical approach even within the physical sciences (Kuhn, 1970; Polanyi, 1958). Attempts to save some aspects of this approach through various forms of philosophical rehabilitation (e.g. Popper, 1959) have also been found wanting (Duhem, 1906/1954; Quine, 1953/1961; Quine & Ullian, 1970). As a result, the pursuit of laws is perceived by many to be the wrong standard against which to judge progress in science, especially when there are many other less limiting standards (e.g. Feyerabend, 1975; Hull, 1988; Lakatos, 1970; Latour, 1987; Laudan, 1977).

If the discovery of abstract laws is the wrong standard against which to judge scientific progress in general, it is a downright silly standard against which to judge progress in social psychology. The Wallachs are misreading the objectives of the discipline when they write that '[e]xperimental social psychologists have generally conceived of themselves as building a scientific system of lawful relationships from scratch' (p. 465). We doubt that more than a handful of social psychologists would characterize themselves as doing this, and we know of no social psychology textbook that includes any discussion of social psychological laws. When they write that 'little knowledge will be gained by attempts to formulate and test lawful

relationships distinctive to social psychology' (p. 466), they echo the tacit assumptions of active researchers. And when they write that 'research aimed at establishing social psychological laws seems at least frequently to accomplish nothing but the confirmation of relationships the confirmability of which is already implied by presuppositions of that research' (p. 468), they criticize a practice that does not, in fact, exist.

What social psychologists do instead, in virtually every empirical investigation we know of, is to attempt to accomplish the sorts of objectives that the Wallachs mention at the end of their essay, when they identify other functions that can be served by empirical research. They note approvingly that '[s]upporting lawful relationships is clearly not the only function experiments can perform' (p. 467). We would rephrase this: it is virtually never the function performed by experiments in social psychology. These experiments serve primarily to test highly probabalistic causal relations between psychological constructs, and to identify the inevitable limits of those causal relations; they do not seek to discover or support laws.

Measurement Models and Conceptual Hypotheses Are Entirely Separate Structures

The Wallachs' criticism depends centrally on the necessity of some means of measuring psychological constructs that cannot be directly apprehended by the senses. Toward making this point they write, 'Although subjective states and dispositions can be identified empirically, this cannot be done without, usually implicitly, relying on conceptualizations or theories of them' (pp. 452–3). This point is surely true.

But the Wallachs' argument proceeds to make some logical missteps. In pursuing the example of anger, they write that we must invoke some theory that 'assumes that there is such an emotion of anger, and that anger may be brought about by insult and result in aggression' (p. 454). The first part of this statement is right: in order to specify and test any hypothesis about anger, one must indeed assume that anger does exist and articulate some operational definition indicating how anger can be measured. But the last part is dead wrong: there is nothing about the conceptualization or operationalization of anger that implies consequences beyond that specific operationalization. A researcher might operationalize anger on the basis of expert coding of facial expressions, and that does indeed imply some tacit theory about causal effects of inner emotional states on facial expressions. But that poses a problem of circularity only if one is testing a conceptual hypothesis about the relation between anger (as measured by facial expressions) and facial expressions. This is not what social psychologists spend time on.

It appears that the Wallachs' criticism is based on an essential confusion between two very different types of theoretical models: (a) a measurement model specifying what a psychological construct is and what observable variables might serve as useful indicators of that construct; and (b) a conceptual hypothesis specifying interesting relations between different psychological constructs. The two types of models are designed for two very different purposes. The measurement model specifies logical relations between the conceptual and the operational levels of analysis. The conceptual hypothesis specifies logical relations between different constructs within the conceptual level of analysis. The two types of models do necessarily 'touch' each other in that they both specify something about a common construct (e.g. anger). But rarely does confidence in one model imply confidence in the other. We can be confident that anger is expressed reliably in the form of certain specific measurable facial expressions, but our confidence in this measurement model cannot make us more confident that anger has a hypothesized causal influence on, say, aggressive social behavior. Similarly, we can be confident in the manner that we operationally define a particular belief, but this cannot make us any more confident that the particular belief has conceptually interesting consequences on other cognitions or behaviors. Thus, the Wallachs are quite wide of the mark when they assert that 'the Belief Principle itself . . . is an integral part of every conceptualization [or theory] of believing' (pp. 458–9). More generally, when these two types of models are logically separated—as they are in just about every test of every social psychological hypothesis—the alleged problem of circularity disappears.

Circularity Need Not Be a Substantial Problem for Progress

The two preceding points reveal that the Wallachs' criticism is based on faulty assumptions about the objectives of social psychological research and about the structure of social psychological hypotheses. These two considerations undermine the thrust of their critique. There are several ancillary elements to their critique that also bear some closer examination. One of these is the implication that any circularity in argument structure renders the argument unsound.

The Wallachs argue that certain poor theoretical arguments, based on circularities or tautologies, lead to a science in which no progress is offered by empirical research. The implication seems to be that, whenever tautologies or circular arguments are present in the development of a hypothesis, the hypothesis is not worth testing. However, logically, this implication is in error. The insertion of a necessarily true argument into a longer argument has no effect whatsoever on the truth value of the entire argument. Consider the formal example below:

        Argument 1
        Prior Theoretical Hypothesis: If A, then B.
        Current Theoretical Hypothesis: C is a case of A.
        Empirical Observation 1:   When A, B.
        Empirical Observation 2:   When C, not B.
        Theoretical Conclusion: Therefore, C is not a case of A.

This argument is well formed and logically true. Now, suppose that part of
the theoretical argument included a tautology. The argument might look like
this:

        Argument 2
        Prior Theoretical Hypothesis: If A, then B.
        Current Theoretical Hypothesis: C is a case of A.
        Another Theoretical Hypothesis: A will occur when A is found to occur.
        Empirical Observation 1:   When A, B.
        Empirical Observation 2:   When C, not B.
        Theoretical Conclusion: Therefore, C is not a case of A.

This argument is equally well formed, and also logically true, although it
contains a bit of foolishness, in the form of 'Another Theoretical Hypo-
thesis'. This foolishness is tautological of course. But this lazy bit of
theoretical development has no effect on the truth value of the argument, the
logical conclusion one should draw from the observation, or the overall
quality of the contribution of the observation. If the circularity, or tautology,
is the fundamental essence of the theoretical argument, then, yes, any given
hypothesis is foolish, and no progress can be made. If, however, the
circularity is just one modest piece of an argument that covers substantial
ground elsewhere, then the insertion of a nearly tautological, or even a purely
tautological, argument causes no harm to the truth value of the argument.

   The Wallachs argue that many studies are based on assumptions that are
necessary to the coherence of the logical and empirical argument of the
studies. This may be true, but it's hardly a sin. Just as the physical sciences
must rely on the assumption that there is an underlying physical reality that
approximates what the theory says, so must social and behavioral sciences
make these assumptions. Physical scientists make assumptions about the
nature of reality that are essentially untestable, and these assumptions are the
core of their theories (e.g. the future will be like the past; laws of physics are
constant across long periods of time). Why should we not think that there is
an underlying and meaningful psychological reality, and use this assumption
as part of our work?

Individual Studies That Are Useless Do Not Substantially
Hinder Scientific Progress

We have argued that the Wallachs' concern with circularity is misplaced,
and criticism of the logical underpinnings of social psychological research is

in error. This doesn't mean that every test of every social psychological hypothesis is a winner. For reasons quite independent of circularity, there are lots of studies that offer almost nothing conceptually new to the field. And so the question remains: just how big a problem does this pose to the discipline as a whole?

The image one gets from reading the Wallachs' paper is that scientific progress in social psychology is stymied by the presence of these studies that test derivative, too-obvious hypotheses and yield results that are largely useless.

But science is not a lonely pursuit. Chomsky (1992) wrote that 'real research is always a collective activity' (p. 100). Science is a collective enterprise, involving collaboration, persuasion, shared perceptions and intellectual battles between groups of scientists who do not share perceptions (Hull, 1988; Kitcher, 1993; Latour, 1987). Scientific progress rarely depends upon the results obtained in individual laboratories or published in individual articles. Watson and Crick's famous paper on the structure of DNA would have been meaningless without scientists all over the world agreeing on its value, and subsequent data bearing them out. What makes an idea prevail is not the pure logical basis of a hypothesis or a study. Instead, it is the scrutiny and adoption by the community of scientists (Hull, 1988). As Feyerabend (1975) has shown, adoption of these ideas or principles need not follow logical principles, even in the vaguest of senses. Scientific progress depends crucially on the work of many people, working in many different places in different ways, with different methods, different biases and different goals. Given that science proceeds as a collective activity, progress is not hindered by the results obtained in individual labs or published in individual articles. The failings of any one scientist, any one study or any one theory are of little importance. There is little harm done by studies with bad logic, bad operationalizations or even bad ideas, as they are less likely to persist in scientists' reasoning about phenomena. Progress in social psychology—as in all sciences—is dictated more by the best research, not the worst.

As a result, there is little to be gained by objecting to the failings of individual studies. If the point is to comment upon the state of a scientific field, there is little purpose served by any selective survey of individual studies. It is the full scope of scientific endeavor and field-wide progress that is the best indicator of success. By this standard, social psychology is progressing remarkably well.

The Proof of the Pudding Is in the Eating: The State of Social Psychology Today

The most convincing response to the Wallachs' concerns is made not by logic-based arguments or appeals to philosophy of science, but rather by a

TABLE 1. Recent developments in social psychology: Social psychology is doing remarkably well!!!

Some recent areas of research that suggest the field is healthy:

Stereotype threat	Social identity
Self-presentation	Affective processing
Stigma and the self	Nonconscious processes
Personal relations	Thought and emotion suppression
Terror management	New models of prejudice
Political psychology	Attachment and relationships
Cross-cultural theories	Altruism
Counterfactual thinking	Shifting standards
Law and psychology	Compliance
Happiness and life satisfaction	Implicit processing in stereotyping
Socially shared cognition	*and much, much more . . .*

simple scan of the scientific and social landscape surrounding social psychology. People both within and outside the field perceive that there have been tremendously interesting discoveries resulting from social psychological studies. These include discoveries about basic psychological processes, and about the important implications of these processes on individuals' lives.

To illustrate, Table 1 lists a number of topics to which experimental social psychological research has made real, substantial and interesting progress within the last decade. Findings on those topics address questions at the very core of the psychological sciences. These topics are of interest not only to psychological scientists, but to people outside the discipline as well.

Results of social psychological experiments continue to be read with interest and applied in real-life settings by individuals working in politics, in marketing, in health, and in many other fields of endeavor. Further evidence of the vitality of the field is revealed regularly by media coverage of the results from social psychological experiments. Social psychological research makes regular appearances—perhaps more so than ever before—on the science pages of the most trusted newspapers in Canada (the *Globe and Mail*) and the United States (the *New York Times*, the *Washington Post*). Our own recent experiences reveals that organs of popular media, both highbrow (e.g. the *New Yorker*, the BBC) and lowbrow (e.g. *People* magazine) are keen to broadcast the results of recent social psychological research findings to a wide audience, who presumably find those results informative.

Thus, when one stands back a little from idiosyncratic deconstructions of the logic of individual social psychological hypotheses, and instead surveys the number of very interesting social psychological findings currently receiving attention within the field and without, the enterprise of social psychology looks remarkably healthy. Sure, there are some poorly conceived theories and boring studies and wasted efforts. And, sure, sometimes it

seems that progress occurs more slowly than it ideally should. But these are descriptions that characterize all the empirical sciences (Horgan, 1996). As such, social psychology is in the same boat as the rest of the sciences—and a fine craft it is.

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